

I must add a few words about specific differences. Some one has recently observed that Mr. Darwin has given no definition of a species. I do not propose to attempt the task. But the majority of botanists demand that a "good species" shall be distinctly marked off from every other by definite and tangible characters. The members of the group may either conform pretty closely to a common type, or exhibit a good deal of variation amongst themselves, and this variation may be sometimes indifferent, sometimes adaptive. Such a variable group is generally considered to contain species "on the make"; but the indifferent variation will be remorselessly, if slowly, brought to book by natural selection. A botanical species is then a discontinuous group marked off by characters which I believe to be adaptive.

I have a strong suspicion that zoologists have a different conception of a species from that of botanists. I once heard Prof. Huxley say roundly, at a meeting of the Linnean Society, that there were no such things as species at all. The subject under discussion was a group of *Salmonide*, and he said that if the forms were arranged in a row, it was a purely arbitrary matter how any one chose to cut it up into species. But the same state of things might be paralleled amongst plants—as, for example, in *Hieracium*. The occurrence of such cases is not incompatible with the fact that the majority of species probably admit of being sharply defined, and are, in other words, discontinuous. This is, at any rate, the case with plants, and I do not see why it should not be equally so with animals. If, however, zoologists cut their species finer, it is intelligible that they may find difficulty in recognising the distinctive characters as adaptive. Prof. Poulton, in the discussion at the Linnean Society, went the length of saying that he saw no objection to giving a name to every distinct form, leaving it to be afterwards decided if it were or were not entitled to specific rank. Such a proceeding, if general, would throw taxonomy into a state of chaos. It has been adopted by a few botanists; but by common consent their writings, though not without a certain interest as studies in variation, have been excluded from serious taxonomic literature. W. T. THISELTON-DYER.

Kew, July 20.

IN his letter, published in NATURE of July 16, Prof. Lankester has formulated with great clearness his views concerning the utility of specific characters; and he explains that his chief object in doing so is to draw attention to certain statements of mine, which he declares to involve a serious logical fallacy. While I am grateful for the courtesy with which Prof. Lankester has tempered his condemnation of my logic, I am still unconvinced; and the point at issue is so important that I am anxious to state, as clearly as I can, what my own position is. I may perhaps conveniently begin by quoting in full a passage from a former paper. Last year I wrote as follows:

"In order to estimate the effect of small variations upon the chance of survival, in a given species, it is necessary to measure *first*, the percentage of young animals exhibiting this variation; *secondly*, the percentage of adults in which it is present. If the percentage of adults exhibiting the variation is less than the percentage of young, then a certain percentage of young animals has either lost the character during growth, or has been destroyed. The law of growth having been ascertained, the rate of destruction may be measured; and in this way an estimate of the advantage or disadvantage of a variation may be obtained" (*Roy. Soc. Proc.*, vol. lvii. p. 381).

Prof. Lankester objects to this passage; and, if I understand him rightly, his objection may be stated in this way:—Admitting it to be proved that variation in a certain dimension, among young animals of a species, is associated with change in the death-rate, so that when this dimension increases the death-rate increases, and when it diminishes the death-rate diminishes; so that by ascertaining the magnitude of this dimension in a young animal you can accurately measure its chance of becoming adult;—admitting this relation to hold through a range of experience sufficient to form the basis of a reasonable induction, you have still no right to say that change in the observed dimension is a cause of the subsequent change in death-rate; for the two observed phenomena, namely the change in the observed dimension and the subsequent change in the death-rate, may alike be due to variation in some unobserved character, which alone is effective in causing change of death-rate.

In other words, you have a phenomenon, namely death rate, preceded invariably by two or more phenomena of structure or function; and these are so associated, that from a known change in the antecedent group of phenomena, affecting always every member of the group, you can infer a change of known magnitude in the death-rate. Under these circumstances, Prof. Lankester thinks it legitimate to pick out one of these antecedent phenomena, and to speak of it as the only effective cause of change in death-rate, the other changes, although equally universal, being merely unimportant concomitants of this one essential change. He further finds something extraordinary in my logical position when I disagree with him, and considers every member of the group of correlated changes which invariably precedes change in death-rate as one of the causes of that change.

I have ventured to restate Prof. Lankester's position in my own words, in order to show what I believe him to mean. If I have in any way misrepresented him, I trust he will forgive me.

My own view seems to me identical with that held by a large number of persons, from Hume onwards; and for that reason I hope Prof. Lankester will not think I am indulging in an "empty wrangle" if I ask whether he accepts the following statement:

"We may define a cause to be an object, followed by another, and where all the objects, similar to the first, are followed by objects similar to the second, or in other words, where, if the first object had not been, the second never had existed. . . . We may . . . suitably to experience, form another definition of cause, and call it, an object, followed by another, and whose appearance always conveys the thought to that other. But though both these definitions be drawn from circumstances foreign to the cause, we cannot remedy this inconvenience, nor attain any more perfect definition, which may point out that circumstance in the cause, which gives it a connection with the effect. We have no idea of this connection; nor even any distinct notion of what it is we desire to know, when we endeavour at a conception of it" (Hume: "Inquiry concerning Human Understanding," § vii.).

When I have spoken of cause and effect, I have always endeavoured to use the words in accordance with the definition given in this passage or in Kant's extension of it; but Prof. Lankester seems to go beyond it. At least, the process of selecting one out of a group of universal antecedents, and calling that one alone the effective cause of the consequent, seems to me to involve precisely that knowledge which Hume and all his followers disclaim. For unless he knows "that circumstance in the cause which gives it a connection with the effect," how does Prof. Lankester pick out that one of the universal antecedents of an event which he chooses to call the cause? Such selection would have been impossible to Hume; and if Mill had regarded it as possible, he would hardly have defined a cause as "the sum total of the conditions, positive and negative taken together; the whole of the contingencies of every description, which being realised, the consequent immediately follows."

It is the assumption of the right to choose one out of a number of universal antecedents, and to regard this as the only cause of the consequent, which I have ventured to call illogical: and since Prof. Lankester has quoted Mill against me, I would ask him to read Mill's opinion of such a proceeding.

The prevalence of this practice in biological speculation tends more than any other habit to that neglect of the real complexity of the phenomena of life which Prof. Lankester himself so justly deprecates. For example, the contraction of the body of an amoeba has been discussed of late years in two ways. Observers, following Prof. Lankester's method, have discussed the question, how much of the body of an amoeba is the effective cause of its contractility? It is possible roughly to divide the body of an amoeba (neglecting the nucleus) into an apparently more solid net-work or sponge-work, and an apparently more fluid substance in the meshes of this sponge-work. The question has been hotly debated, which of these two substances should be regarded as the essentially contractile element, the other being an unimportant concomitant. Each alternative has had its advocates, and neither party has convinced the other. Readers of NATURE are aware that a short time ago Prof. Bütschli attacked this question from the standpoint which I am here advocating: that is to say, he regarded each of the substances, invariably antecedent to contraction, as one of the causes of the contraction. By considering the changes in the relation between the two, Prof. Bütschli was at least enabled to make a dead

model which imitates with remarkable exactness the phenomena of amoeboid movement; while the suggestion that such movement falls into the same category as the change in surface-tension at the boundary between two not-living liquids with change in the constitution of either, is a most important step in the "explanation" of contractility in general.

Here then is a phenomenon which had for years been rendered more obscure by the attempt to fix upon one of its two universal antecedents as its effective "cause"; while some kind of explanation was at once forthcoming when both antecedents were taken into account. It would be easy to multiply examples of this kind; but perhaps the foregoing may suffice.

I would only now reiterate my hope that in trying to make plain my own position I have not in any way misrepresented that adopted by Prof. Lankester.

W. F. R. WELDON.

Marine Biological Laboratory, Plymouth, July 18.

IT appears to me that Prof. Weldon's argument, referred to in NATURE of July 16 (p. 245), is accurately represented in the following illustration. It might be an established fact, although it is not in reality, that there was a constant correlation between baldness and short-sightedness. Suppose that it were so, and that in a country where conscription was enforced, short-sighted men were exempt from military service; that is to say, let us suppose that a test was applied to the eyes of all men at a certain age, and that those whose vision was not normal were rejected and allowed to return to the peaceful pursuits of civilian life. These rejected men would, on the hypothesis, be all more or less bald, and according to Prof. Weldon's position, it would be quite as correct to say that they were not in the army because they were bald, as to say they were rejected on account of myopia. Now it is quite true that the officers of the army medical staff might save themselves trouble by rejecting all bald-headed men, because, on the hypothesis, all such men would be short-sighted; but it would be obviously wrong to conclude that a good development of hair was essential to military efficiency.

Prof. Weldon argues that it is enough to prove that individuals of a species are selected according to a certain character, and that it is unnecessary to discover whether survival depends directly on this character, or on some other with which it is correlated. He seems to have concentrated his attention on the attempt to demonstrate directly the occurrence in nature of individual selection, in this peculiar sense, and to be temporarily indifferent to all other questions.

Prof. Lankester suggests that specific characters would be explained if it were proved that they were correlated with adaptive characters. It is of course perfectly true that if there were such constant correlations, then the survival of adaptive variations would involve also the survival of the indifferent characters connected with them. But the difficulty is to prove that in many cases there are *any* important differences of adaptation between allied species. It is easy enough to define the specific differences and specific characters; but to find any differences which correspond to differences in the mode of life, is often exceedingly difficult. It is true we find in most cases some differences in the conditions of life of closely allied species, but we do not usually find peculiarities of structure which can be said to be adapted to those differences. Who, for instance, can say what adaptation is present in the pilchard or sprat differentiating either from the other or from the herring? The question, therefore, is not whether indifferent specific characters are correlated with useful characters, but whether species of a single genus are distinguished from one another by any characters which can be proved to be useful or adaptive. The tongue and hyoid of the woodpeckers are beautiful adaptations; but are there any differences of selection value between one species of woodpecker and another? The denial of the utility of specific characters means, not merely that some specific characters are indifferent while others are adaptive, but that adaptations are not in the great majority of cases distinctive of species at all. Therefore, as the late Mr. Romanes often ably demonstrated, natural selection is not a theory of the origin of species, but only a theory of the origin of adaptations. The further objection, that a theory of selection is only of secondary importance in comparison with a theory of the origin of variations, I will not enter upon on this occasion.

J. T. CUNNINGHAM.

College of Surgeons, July 17.

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### The Position of Science at Oxford.

WILL you allow me a few lines in which to express my entire agreement with your recent article on this subject, if only to emphasise the fact that I am not the author of the article, and that the opinions there expressed are not those of an isolated individual. The reason for the comparative neglect of natural science at Oxford is that, however well-disposed some individuals may be, the college tutors and lecturers as a rule dislike it. They dislike it for two reasons. First, because it cannot be taught in the college parlours called lecture-rooms; and second, because they are, as a rule, ignorant—owing to their own defective education—of the nature and scope of the immense field of study comprised under the head "natural science." They do not know either the enormous educational value of natural science, or its vital importance to our national life and development.

And lastly, if they did know, there is no conceivable motive which could operate so as to induce them to sacrifice some of the rewards and educational domination, which are at present enjoyed by the long-established classical and historical studies, to newer lines of work in which the present beneficiaries and their academic offspring can have no share.

The situation is a "dead-lock," and only an intelligent Parliamentary Commission (if such is possible) can put matters on to a fair and healthy basis. Probably the scandal of the present paralysis of our beloved Oxford will have to become even greater and more outrageous than it is at this moment, before the necessary remedy is applied.

But happily the vitality of Oxford is indestructible. The misused and monopolised resources of Oxford will assuredly some day be devoted to the true purposes of a great University.

E. RAY LANKESTER, Linacre Professor, Oxford.

THERE are some points in the article on this subject in NATURE of July 9, which call for comment. The defects pointed out are not, I believe, due to the causes mentioned by your correspondent. The fault lies mainly in the public schools. The lower forms of public schools are, as a rule, mainly classical, the division into sides, classical, modern and science, only beginning when a boy has finished about half his school career. The choice of sides is chiefly left by the parents to the masters, and since in the lower forms these masters have, as a rule, little sympathy with any kind of work which is not purely classical, boys of ability are drafted as a matter of course into the classical side. The boys who enter the science side are often the failures of the classical side, and unless special care is taken by the science masters, even they are kept at classics until it is hopeless to make them into respectable science scholars. Naturally there are many exceptions; some clever boys have enlightened parents, and others, early developing a taste for scientific matters, persuade their parents to allow them to give up the dead languages. There are also some classical men who admit that other subjects than their own have educational value. But the rule is for the able boy to be kept at classics, while his less favoured brother is sent to science. I know that this is the case at the five public schools with whose working I am familiar, and I have little doubt that the science masters of other public schools have the same experience. Occasionally able boys are recruited from the modern side, and it is these boys who are practically shut out from Oxford. However small the knowledge of Greek required for passing responsions may seem to a classical man, it is no light matter for a boy who has it all to learn in little more than a year, and who has much other work to do during the time. At Cambridge the necessary knowledge of Greek is almost nominal, and it is a pity it is not abolished altogether. If both Universities would substitute a good knowledge of German—so necessary for every scientific student—for the very imperfect and quite useless modicum of Greek which they now require, it would result in a great saving of time to many science students, and ultimately in raising the science standard at both Universities.

H. B. BAKER.

IN your article on "Science at Oxford," in NATURE for July 9, you say: "It may be objected that every public school has one or more science masters of tried capacity, and that science is a compulsory subject in nearly all."

The first part of this statement may be correct, but I venture to demur to the second. Certainly at one school I could name,